1861MNRAS..22....1H

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXII. *November* 8, 1861. No. 1.

Dr. LEE, President, in the Chair.

Col. T. Schaffner, Kentucky, U.S.A.,
was balloted for and duly elected a Fellow of the Society.

Extract of a Letter from Professor Hansen to the Astronomer Royal, dated Gotha, 1861, *Oct.* 18.

My principal labours advance progressively, and I work at them with pleasure. Events in the last summer have indeed occasioned some delay, and my other various avocations also often create cessations, but nevertheless I proceed. The working out of the new terms depending upon the planets, which I mentioned in my last letter, has occasioned a considerable addition to the labour of the calculation of the secular variation of the Moon's mean longitude, which I have not yet quite completed. I must in fact execute in regard to these terms as great a labour as in the calculation of the lunar perturbations themselves. I must carry on the approximations until the values at last substituted come out again. These terms are far greater than I suspected at first, and the circumstance that here the coefficients of the terms which have very small divisors become remarkably large particularly increases this labour. In the lunar perturbations themselves the analogous terms have only small coefficients. I am, I may say, perfectly

1861MNRAS..22....1H

convinced that the value of the secular variation which I have before calculated from theory and assumed in the Lunar Tables must again come out, although I have considerably changed the method of calculation.

In the calculation of the secular variation of the mean longitude, I have set out with the proposition that the element which I have always denoted by Ξ has no influence over it. This proposition I have already proved, even to the cube of the disturbing forces, in my third memoir upon the perturbation of the small planets, and the proof holds also for the Moon. Further, the numerical equations of condition have shown me that this proposition is true even for the higher powers of the disturbing forces, at least to a considerable extent.

I have calculated anew the principal part of the perturbations of the longitude, of the radius vector, and of the latitude, and obtained such an agreement with the coefficients used by me in the Lunar Tables, and derived from the previous theoretical calculations that the differences in the individual coefficients amount only to a few hundredths of a second of arc. A new calculation of the secular variation of the motion of the perigee is also finished, and the deviation of the coefficient thus found from that of the Lunar Tables scarcely amounts to $0''\cdot4$. Now this coefficient, multiplied only by $2\,e$, enters into the longitude, and as $2\,e$ is nearly $=\frac{1}{9}$, this difference is of no greater significance than $0''\cdot04$ in the coefficient of the secular variation of the mean longitude.

As I have brought such accuracy into my theoretical determinations of the various parts of the Lunar Theory, it of course follows that I do not need to deem the cry which some have raised concerning empiricism finding place therein deserving of the least notice.

In order, however, as much as possible to comply with every reasonable desire concerning the early publication of my theoretical determination of the coefficients of the Lunar Theory, I have occupied myself, for some short time past, with putting together and sending to the press my calculation of the principal perturbations, which, as stated above, has been executed afresh. As soon as possible will follow, also, the publication of the calculation of the smaller coefficients. You will ask perhaps why I then do not publish the first calculations, which were finished long ago, before the completion of the Lunar Tables? and I answer, that I executed the last approximations in that calculation by substitution of the differences with the preceding approximation, and that therefore, by the publication of these calculations, a complication would arise which could only be made intelligible with great difficulty.

In the publication the result must proceed from *one* fountain; and it was necessary, therefore, that a new approximation should be calculated, with the full values of the coefficients. Only in this manner can I make myself clear, and place every

1861MNRAS..22....1H

one in a position to be able to examine with ease the individual parts of my calculation.

With regard to the two small errors in the Lunar Tables which you communicate to me, they have arisen in this manner:—Page 86, Arg. 37 and (a) have, according to my present re-calculation, been calculated by my former computer, who has made so many errors, with an incorrect coefficient of $(c-18)^3$. The influence of this error, however, is so insignificant that it can, even in the year −800, produce an effect, at the most, of $1''\cdot8$ upon the Moon's longitude, and, as a matter of course, still less in subsequent centuries. I consider, therefore, its correction to be superfluous. Page 77, Arg. 10, Year −500, 56·619 was written by mistake instead of 56·691.

Note by the Astronomer Royal.

The small errors to which Professor Hansen alludes in the last paragraph were pointed out by Dr. Hincks to the Astronomer Royal, in a letter dated 1861, Sept. 16. Dr. Hincks remarks, "The whole columns 37 and (a), p. 86, are miscalculated, the error in the former being proportional to the cube of the number of centuries before 1800. $\overset{37}{9\cdot1823} + \overset{(a)}{275}$ ought to be $9\cdot1350 + 330$; and so in other instances down to 1600, when the error disappears. I happened to notice a minor error in p. 77, −500, Arg. 10, where 56·619 is printed for 56·691. This, however, produces no sensible error."

On a Result deduced by M. D'Abbadie from Observations of the Total Solar Eclipse of 1860, *July* 18. By the Astronomer Royal.

In the *Compte Rendu* of the French Academy for the meeting of 1860, November 12 (tome li.), M. Antoine D'Abbadie has given an account of his observations of the eclipse of July 18, and in particular of his measures of the height of a certain red prominence A, to which my present remarks are confined. The telescope employed had an aperture of $72^{\text{mm}}\cdot5$ (2·9 inches nearly), a focal length of 800^{mm} (32 inches nearly), and was used with a magnifying power 47. The micrometer consisted of a glass plate in the field of view, bearing a net of equal divisions, which divided the whole field into squares of $51''$ each side.

Omitting all observations except those which I have specified, M. D'Abbadie's record is as follows:—

1861MNRAS..22....1H

No. de l'Observation.	Temps Moyen.	Phénomène Noté.	Angle reduit en hauteur.
	h m s		′
3	2 47 45	Hauteur de A, 1·3 division (probablement 2·3).	1·9
5	2 48 26	Hauteur de A, 1·3 division, dont une en diagonale.	1·4
7	2 49 27	Hauteur de A, 0·7 division.	0·6

upon which he founds the following inferences:—

"En calculant le mouvement relatif des deux astres par les Tables de MM. Hansen et Le Verrier, et en comparant les différences à celles qu'on deduit de mes observations relatées ci-dessus, on obtient les résultats suivans,—

Numeros.	Diminution observée dans la hauteur de la prot. A.	Mouvement correspondant calculé de cette partie du disque.	Mouvement maximum calculé dans la direction même du mouvement relatif des deux astres.
	″	″	″
De 3 à 5	30	15·0	18·3
De 5 à 7	52	22·4	27·2

"Ici, la decroissance observée est plus de deux fois celle qui serait produite par le simple mouvement relatif des deux astres."

We owe it entirely to the candour of M. D'Abbadie and to the completeness of his published statement, that we are in any way enabled to criticise his conclusions. I trust, therefore, that, in expressing my inability to agree with M. D'Abbadie as to the method of treating his observations and as to the results to be drawn from them, I may be understood as offering to him my thanks and those of other persons interested in this discussion, for the clear and full account of his original observations, from which we are all enabled to frame our own deductions.

The conclusion of M. D'Abbadie regarding the discordance between the observed decrease of the prominence and the computed change in relative positions of the Sun and Moon, rests *entirely* upon his arbitrary correction of the first observation; where his recorded measure was 1·3, which measure he has changed with the simple remark "probablement 2·3", and has used so changed in the subsequent computations. There is nothing to lead to the supposition that any circumstance attending the observation itself called for a change. It has been made, as far as we can judge, simply because the progress in the measured height of the protuberance between 3 and 5 is inconsistent with that between 5 and 7: it has, in fact, the wrong sign.

But if we are permitted to make such changes, where shall we stop? Why not alter the measure 5 instead of 3, which would have led to a discordance in the opposite direction? I conceive that it is much safer to use the observations as they

1861MNRAS..22....1H

stand, to extract from them the best result which mathematics enable us to draw, to form the numerical values of the residual errors, and to judge whether these can properly be attributed to error of observation.

Adopting, then, for observation 3 the *original* value 1·3, and considering the diagonal measure in observation 5 as 1·4, we have the following measures of the height of the prominence:—

No.	Mean Time.	Height of A in divisions.	Height of A in seconds.
	h m s		
3	2 47 45	1·3	66
5	2 48 26	1·7	87
7	2 49 27	0·7	36

Let x be the height of the prominence at $2^h\ 47^m\ 0^s$, y the decrease in its height for 1^m. Then we have

$$x - y \times 0{\cdot}75 = 66$$
$$x - y \times 1{\cdot}43 = 87$$
$$x - y \times 2{\cdot}45 = 36$$

Treating these by the method of Probable Errors, we obtain the equations

$$3x \quad - y \times 4{\cdot}63 = 189{\cdot}0$$
$$x \times 4{\cdot}63 - y \times 8{\cdot}60 = 262{\cdot}1$$

by solution of which, $x = 93{\cdot}8$, $y = 20$.

The rate of change for 1^m, therefore, inferred from the observations, is 20″, agreeing sensibly with 22″ given by the computed relative motion of the Sun and Moon.

If we substitute for the formula $93{\cdot}8 - 20 \times t$ (where t is expressed by fractions of a minute) in each of the observations, we obtain

No.	Computed Height of Prominence.	Observed Height of Prominence.	Excess of Observed Height.
	″	″	″
3	78·8	66	−12·8
5	65·2	87	+21·8
7	44·8	36	− 8·8

From my own experience in observations of eclipses, I conceive that these "excesses," though very large, are not beyond possibility. The second measure (in which it seems to have been necessary to resort to a comparison with the diagonal of the square, I can scarcely imagine why) was evidently made under unfavourable circumstances; and the power of the telescope was low.

I think that M. D'Abbadie's inference is not sustained by his observations.

Royal Observatory, Greenwich,
1861, *October* 22.

1861MNRAS..22R...6H

Total Solar Eclipse, 1861, *Dec.* 31.

By J. R. Hind, Esq.

The following Table, exhibiting the path of the Moon's shadow over the African continent, in the eclipse of 1861, Dec. 31, has been derived from accurate calculations founded upon the Tables of Le Verrier and Hansen, assuming the diameters of Sun and Moon as there adopted:—

Greenwich Mean Time.	N. Limit.		Central Line.		S. Limit.		Duration of Totality on Central Line.
	Long.	Lat.	Long.	Lat.	Long.	Lat.	
h m	° ′	° ′	° ′	° ′	° ′	° ′	m s
2 33	18 13 W.	14 30 N.	18 8 W.	14 11 N.	18 2 W.	13 52 N.	1 43
2 38	16 24	15 36	16 19	15 18	16 14	15 0	1 38
2 43	14 28	16 49	14 23	16 32	14 18	16 14	1 33
2 48	12 21	18 10	12 17	17 53	12 13	17 35	1 27
2 53	10 1	19 40	9 58	19 23	9 54	19 6	1 20
2 58	7 23	21 21	7 21	21 5	7 18	20 49	1 13
3 3	4 18	23 19	4 17	23 3	4 16	22 47	1 5
3 8	0 32 W.	25 40	0 33 W.	25 25	0 33 W.	25 10	0 56
3 10	1 17 E.	26 45	1 15 E.	26 31	1 14 E.	26 16	0 52
3 12	3 22	27 59	3 19	27 45	3 17	27 32	0 48
3 14	5 50	29 25	5 47	29 12	5 44	28 58	0 43
3 15	7 18	30 15	7 14	30 1	7 10	29 48	0 40
3 16	9 0	31 10	8 54	30 57	8 48	30 44	0 37
3 17	11 2	32 16	10 53	32 2	10 46	31 49	0 34
3 18	13 42 E.	33 39 N.	13 29 E.	33 24 N.	13 17 E.	33 9 N.	0 31

Nautical Almanac Office,
1861, *Nov.* 2.

Observations of the Solar Spots. By Rev. F. Howlett.

Mr. Howlett presented to the Society a further series of drawings of solar spots. The following is extracted from the letter, dated St. Augustine's Parsonage, Hurst Green, Sussex, Nov. 5th, 1861, which accompanied the drawings:—

"The present series, continued from Nov. 15th, 1860, and brought down to Oct. 31st of the present year, consists of about seventy-three drawings of the whole solar disk, as magnified about 30 diameters, besides upwards of 200 separate spots, grouped or single, as amplified from 120 to 150 diameters.

"I may be permitted to remark that the wonderful commotions exhibited on the solar surface during the years 1859 and 1860 have scarcely, I think, been equalled at any period during the year which is now drawing to a close. These commotions,